voyage of circumnavigation, you found Gallia to be composed—a substance to which your geological attainments did not suffice to assign a name.'

"The professor took the cube, and, on attaching it to the hook of the steel yard, found that its apparent weight was one kilogramme, and four hundred and thirty grammes.

"'Here it is, gentlemen; one kilogramme, four hundred and thirty grammes. Multiply by seven; the product is, as nearly as possible, ten kilogrammes. What, therefore, is our conclusion? Why, that the density of Gallia is just about double the density of the earth; which we know is only five kilogrammes to a cubic decimetre. Had it not been for this greater density, the attraction of Gallia would only have been one-fifteenth instead of one-seventh of the terrestrial attraction.'

"The professor could not refrain from exhibiting his gratification that, however inferior in volume, in density, at least, his comet had the advantage over the earth."

We have given this long extract to show the pleasant way in which, in this latest form of French light literature, amusement is combined with instruction. It would not be fair to the book to say more of the plot or of the *dénouement*.

We have dwelt especially upon Jules Verne's latest book, but equal praise must be given him for all those we have named. A boy, for instance, who had read how the frozen island in the "Fur Country" was kept together by Dr. Black's device, would at once understand the *rationale* of Pictet's and Cailletet's recent splendid work, to say nothing of the physical geography he would have gradually absorbed in following the strange adventures recounted in that volume.

We are glad to have such books to recommend for boys' and girls' reading. Many young people, we are sure, will be set thirsting for more solid information.

OUR BOOK SHELF

The Geometry of Compasses; or, Problems Resolved by the mere Description of Circles, and "the Use of Coloured Diagrams and Symbols." By Oliver Byrne. (London: Crosby Lockwood and Co., 1877.)

THIS is only our old friend, "La Geometria del Compasso di Lorenzo Mascheroni" (Paris, 1797), decked out in the manner we have indicated in the quoted portion of the title. The order of sequence has been departed from, but this is not a material point. The constructions are the same and the proofs the same with, we believe, one exception, in which case we give the preference for simplicity to Mr. Byrne.

There are twenty problems, which are in most cases given in duplicate, first construction and figure in colours, then proof and unadorned figure on the next two pages.

The merits and nature of Mascheroni's work are well known; hence the present work, for reasons given above, is good. But we cannot call this Mr. Byrne's book. Problem XX., which is the last, is an elegant construction for dividing the circumference into seven equal parts by plane geometry. But for this the compiler is indebted to an able mathematician, Dr. Matthew Collins. The book is very neatly and correctly got up, and for frontispiece has a hand with a pair of compasses transferring a given length.

Proceedings of the American Philosophical Society. Vol. xvi., No. 99. January to May, 1877.

PROF. COPE has several noteworthy papers in this part: one, on the Batrachia of the coal-measures of Ohio, describes the new genus, Ichthycanthus, and the new species of Leptophractus and Tuditanus. He also describes remains of a Dinosaurian from the trias of Utah; the humerus is one of the longest, and distally the most contracted known in the group. These remains are the first discovered fossils in the triassic beds of the Rocky Mountain regions. Another valuable paper is on the brain of Coryphodon. One of the longest contributions will be much esteemed by geologists, viz., Mr. Ashburner's measured section of the palæozoic rocks of Central Pennsylvania (Huntingdon County), a section extending vertically through 18,394 feet. A very valuable series of physiological experiments is recorded in a paper by F. L. Haynes, on the asserted antagonism between nicotin and strychnia. Philology is well represented by a paper on the Timucua language, by Mr. A. S. Gatschet; this language, formerly spoken in Florida, appears to be the oldest within the American Union of which writings of some extent are preserved.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Radiometer and its Lessons

WITH reference to the controversy between Mr. Stoney and Mr. Osborne Reynolds about the laws of the conduction of heat in gases, it seems desirable to call the latter gentleman's attention to the fact that neither Clausius' nor Clerk Maxwell's investigations, as published in the *Philosophical Magazine*, affect the controversy between them.

The latter, in his papers in the *Philosophical Magazine*, vol. xxxv., lays particular stress upon the fact that he supposes the motions of the molecules to be uniformly distributed in every direction. He says, however, on page 188: "When one gas is diffusing into another, *or when heat is being conducted through a gas*, the distribution of velocities will be different in the positive and negative directions instead of being symmetrical, as in the case we have considered." From this theory of the uniform distribution of velocities he deduces the formula (29), (31), and (32), as he numbers them, and to which he subsequently refers. On page 214 he gets an equation (143) which represents the transference of heat through the medium, and says: "The second term contains quantities of four dimensions in $\xi\,\eta\,\zeta$, *whose value will depend upon the distribution of velocity* among the molecules. *If* the distribution of velocity is that which we have proved to exist when the system has no external forces acting on it, and when it has arrived at its final state we shall have by equations (29), (31), (32) . . ." certain results from which he deduces his equation for the conduction of heat in gases.

When he says "has arrived at its final state" it is evident from his reference to the equations that he means the state of a gas in which neither diffusion nor conduction of heat nor currents of any kind are going on. It will thus be seen that his final result is only a first approximation and could not possibly be expected to hold within distances comparable with the mean length of the path of a molecule between two encounters.

Clausius in his paper as translated in the *Philosophical Magazine* for June, 1862, does suppose a distribution of velocity among the molecules of such a kind that the velocity and number of molecules moving in the positive and negative directions is different, but assumes the mean between them to be the same as the number moving in a direction normal to the direction of the transference of heat. This is evident from the fact that what he practically does is to assume that the number of molecules moving in a direction making an angle θ with the direction of transference of heat can be expressed by a formula of the form—

$$n = n_0\,(1 + e\cos\theta),$$

for he neglects e^2 throughout his investigation. In this form it is evident that n_0 is the number when $\theta = \frac{\pi}{2}$ and is the mean of the values $n_1 = n_0\,(1 + e)$ and $n_2 = n_0\,(1 - e)$, which represent the numbers going towards and from the points of high tempera-

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ture. From the fact that c^2 enters into the investigation at all it is evident that this is only an approximation to the true distribution. In accordance with this Mr. Stoney has shown conclusively that in a compressed Crookes's layer the number of molecules moving parallel to the direction of the transference of heat is greater than the number of those moving in any direction normal to it, so that the expression Clausius derived from his assumption cannot be considered as expressing the whole state of affairs.

It is remarkable that to this order the expression for the pressure on *any* plane is the same, but Clausius gives another term in his expression for the pressure on a plane normal to the direction of transference of heat to which he attaches, indeed, only an indefinite coefficient because it is of the order c^2, and he was purposely neglecting quantities of that order. He might have prophesied, however, from the existence of such a term that at distances comparable with c a force would be manifested such as Mr. Crookes has since discovered. Now this c is by definition a quantity of the order of the length of the mean path between successive encounters, and hence these terms, varying with c^2 would become of importance at distances comparable with the length of this mean path.

I believe, then, that I have shown that neither Clausius nor Clerk Maxwell have considered the case in dispute between Mr. Stoney and Mr. Osborne Reynolds, and that as far as their investigations bear upon it they tend very much to strengthen Mr. Stoney's case. I have also shown that Clausius was on the point of anticipating both Crookes's force and Mr. Stoney's explanation of it. GEO. FRAS. FITZGERALD

Trinity College, Dublin

Prof. Eimer on the Nervous System of Medusæ

SOME of your readers may remember that a few months ago I published in NATURE an abstract of a lecture which I had delivered at the Royal Institution on "The Evolution of Nerves." In this lecture I mainly treated of my recent researches on the nervous system of *Medusæ;* and stated, among other things, that I was the first to publish the observation concerning the paralysing effect of removing the margins of nectocalyces.[1] Within the last few days, however, I have received a communication from Prof. Eimer, of Tübingen, informing me that he has the right to claim priority as regards the publishing of this observation. I therefore send you this note in order that I may rectify the injustice which I previously did to Dr. Eimer in your columns.

The facts of the case are simply these: Dr. Eimer made his observation a few months later than I made mine; but, as he communicated his observation within a few weeks after he had made it to the *Physikalisch-medicinischen Gesellschaft zu Würzburg*, his publication preceded mine. He has therefore the right to claim priority as regards this observation, and also as regards some further physiological experiments by which he followed it up—all of which I have been careful to detail in my Royal Society publications.

So much in justice to Dr. Eimer. In justice to myself I must now explain that, although, since the publication of my Croonian lecture in 1875, I have been aware that Dr. Eimer's work was independent of mine, it is only within the last few days I have learned from him that the publication of his work was prior to mine. The reason of the ambiguity on this head is explained in a newly-published memoir by Dr. Eimer, where it is stated that his previous memoir, having been published in the *Würzburg Verhandlungen* without its proper title-page, the initials "*d. J.*" ("this year"), which occur in the paper itself, refer, not to the date on the volume, but to the year preceding. My prolonged ignorance concerning Dr. Eimer's claim to priority, has, therefore, not been due to any fault on my part; and as in all my previous publications on this subject I have spoken of Dr. Eimer's work as subsequent to my own, I may here add that I think the fact of his having been so long in acquainting me with the true standing of the case, displays a laudable spirit of indifference on his part to the matter of mere priority.

GEORGE J. ROMANES

18, Cornwall Terrace, Regent's Park, N.W.

Mr. Crookes and Eva Fay

AFTER Mr. Cooper's courteous explanation which appeared in last week's NATURE (p. 183), I gladly exonerate him from blame.

To the publication of my letter in the *Banner of Light*, if Mr. Cooper thought it likely to do Eva Fay any good, I have no ground of complaint; but what I did, and do now, protest against, is the unauthorised publication of a lithographed *facsimile* of my letter in such a manner, and with such surroundings, as to leave no doubt that the intention was to throw discredit on my testimony as a trustworthy experimentalist.

I am glad to find that Mr. Cooper was no party to this breach of etiquette, and I willingly withdraw any expressions in my letter in NATURE (vol. xvii. p. 43) which may appear to reflect on him.

As a fitting climax to this controversy, may I request you to publish the subjoined letter from Eva Fay, which appeared in the *Banner of Light* for December 22 last?

London, January 7 WILLIAM CROOKES

"To the Editor of the *Banner of Light*, Boston, U.S., December 22

"I WISH to state a few facts in reference to an article in your paper of December 8, referring to myself, in a letter of Mr. Crookes on Dr. Carpenter's attack.

"First, it is untrue that Mr. Crookes gave me a letter speaking of the spiritualistic nature of my manifestations, and referring to Fellows of the Royal Society. The only letter, to my knowledge, that Mr. Crookes ever wrote regarding my mediumship (with the exception of the one written to Mr. Cooper) appeared in the London *Daily Telegraph*, and other journals, March 11, 1875.

"Second, in reply to Dr. Carpenter's statement that an offer was made by my managers in May, 1875, of an equivalent sum of money for me to 'expose the whole affair,' I will now say to Dr. Carpenter, as I did to my managers, *I have nothing to expose.*

"I am in receipt of a letter, dated November 18, 1877, asking me if I will fix a price to visit England under the title of an 'Exposée,' and show how I am supposed to have hoodwinked members of the Royal Society.

"My reply was as follows:—'As poor as I am, and as clever as I am supposed to be by Dr. Carpenter and others, I am obliged to decline your tempting proposition to replenish my exchequer by attempting impossibilities. I sincerely hope to be able to maintain myself and child in a more honourable occupation.' "ANNIE EVA FAY

"Akron, Ohio, December 10, 1877"

Volcanic Phenomena in Borneo

MR. WALLACE, in his work on the "Geographical Distribution of Animals," has the remark that no volcano, active *or extinct*, is known to exist within the area of the island of Borneo, notwithstanding that it is almost environed by a volcanic belt in full activity at a short distance. In fact, it seems to be generally understood that this vast island now represents, and has continued to represent for long past time, a perfectly quiescent area in so far as manifestations of subterranean energies are concerned. This view is doubtless strictly correct in regard to the existence of any volcanic vent which is now in action, or which has been so within the historical period; but it would be erroneous to deduce from it, as seems natural to do at first sight, the inference that the area is one of entire quiescence, or that it has been so free from volcanic action in any but the most recent times.

Speaking solely with reference to the north-west district, it may be observed that shocks of earthquake have been recorded more than once by credible witnesses during late years, viz., one in June, 1874, a second in June, 1876, and two more in July, 1876. These were recorded the first in Sadong, the three others in Saràwak. According to native testimony, slight shocks are by no means rare, and a severe one is particularly held in remembrance, which took place seventy or eighty years ago, and was accompanied by "a rain of ashes." Seismometrical observation would probably show that they are very frequent. These shocks seem to indicate that the island is directly affected by the proximity of the volcanic band above referred to.

As for the period of time preceding the historical epoch, there are not wanting signs that this part of Borneo was the theatre of a display of considerable volcanic energy, and it has yet to be shown that its date of activity was anterior to the deposition of the sandstone conglomerate formation of the country, which is, with the exception of very recent deposits, the most modern of the stratified rocks of this part of the island, it having been assigned—I know not with how much truth—to a later tertiary

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[1] I first published this observation in a note to NATURE, which appeared in November, 1874.

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